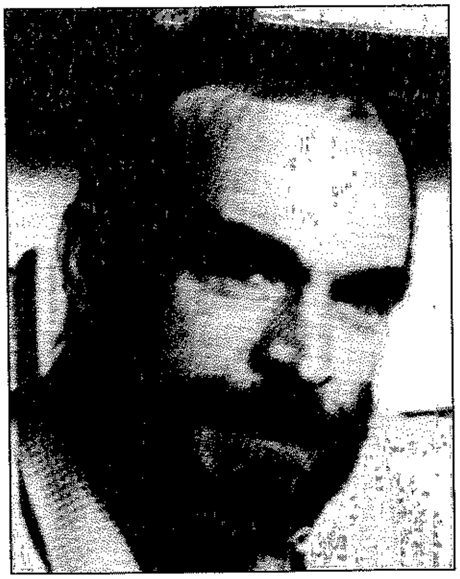
In a 1987 joint paper with Thurston, "Pinching constants for hyperbolic manifolds", one finds two essential constructions for negative curvature that work in every dimension ≥ 4. In both constructions one starts with a compact space form M of hyperbolic type, i.e., the sectional curvature is constant and is equal to -1. Consider in M a totally geodesic submanifold of codimension 2 (i.e., a submanifold N in which geodesics starting in M and tangent to N remain in N). Look now at cyclic coverings of M ramified along N. It is not too hard to endow such a covering with negative curvature, and one can even control the *pinching*, the ratio $\sup K / \inf K$. MG studies the volumes of these objects. A major result of the book [1], *Manifolds of Nonpositive Curvature*, furnishes bounds for the volume as a function of the pinching. This construction yields manifolds whose topology can differ strongly from that of a space form. In the first type of example one can show that the pinching can be as close as desired to 1. Hence the conclusion: for any ε there exist manifolds with curvature in $[-1-\varepsilon, -1+\varepsilon]$, of bounded diameter, that do not admit a metric of constant negative curvature.

A second construction enables MG to obtain examples of a complementary type: for every ε with $0 < \varepsilon < 1$, there exist manifolds of negative curvature that do not admit a metric with curvature in the range $[-1, -1+\varepsilon]$. This result is hard to prove but essential to the understanding of negative curvature. The proof uses the technique of diffusion of cycles discussed in Part I.

Conclusion

If MG has a muse, it is not the axiomatic one of Euclid. MG is instead guided by concepts such as softness versus rigidity, computability, physical reality of objects, etc. In particular, when talking about results, he is concerned with the robustness of the invariants used. His other principle is to avoid empty generalization: "Many theorems are not interesting if one cannot produce examples where the result is not already there". From this point of view the Filling paper [8] is exemplary. In case some of his results do not meet the above criterion, he adds, "Then put them in what is now called foundations."

We have seen time and again that MG's papers are like icebergs: most of the results lie under the surface and are accessible only to exceptional mathematicians who are willing to devote their time to them. So why does MG not write his results in detail? We think that the best way to answer this and other questions is to let MG speak for himself: "Checking in full detail the proof in my head was already so painful that I was left with no energy for more." Let us also quote what he says in a expository paper of 1992, "Stability and pinching":



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> The results we present are, for the most part, not new and we do not provide detailed proofs (these can be found in the papers cited in our list of references). What may be new and interesting for non-experts is an exposition of the stability/pinching philosophy which lies behind the basic results and methods in the field and which is rarely (if ever) presented in print (this common and unfortunate fact of the lack of an adequate presentation of basic ideas and motivations of almost any mathematical theory is, probably, due to the binary nature of mathematical perception: either you have no inkling of an idea or, once you have understood it, this very idea appears so embarrassingly obvious that you feel reluctant to say it aloud; moreover, once your mind switches from the state of darkness to the light, all memory of the dark state is erased and it becomes impossible to conceive the existence of another mind for which the idea appears nonobvious).

Finally, for those who want to know more about MG's process of discovery, we end with the following quotation of his response to his being awarded the AMS Steele Prize in 1997 (*Notices*, March 1997). The response analyzes the results of [10], "Pseudo-holomorphic curves in symplectic manifolds", a paper that was discussed in Part I:

> I saw the light when struggling with Pogorelov's proof of rigidity of convex surfaces where he appeals to the Bers-Vekua theory of quasi-analytic functions. There was nothing seemingly complex-analytic in the linearized system written down by Pogorelov, and